

BELLCOMM, INC.

1100 Seventeenth Street, N.W. Washington, D. C. 20036

SUBJECT: A Rationale for Priority Rating
MSF Experiments in the Definition
Phase - Case 710

DATE: April 10, 1968

FROM: F. G. Allen
A. R. Vernon

ABSTRACT

A rationale is developed for assigning priorities to manned space flight experiments still in the definition phase. All experiments are scored on their potential worth to the space program and on three merit factors: suitability for manned flight, likelihood of success and cost effectiveness. A numerical scoring system has been worked out and the method has been tested empirically on a set of over 100 experiment proposals in all disciplines. Despite shortcomings inherent in any such system, the results were reasonable and very useful in providing both an overall ranking and a detailed view of strengths and weaknesses of each experiment. The priority ranking is, of course, only one guide. It does not provide an assessment of program balance, completeness and experiment schedule which must all be considered for an optimum allocation of funds.

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MEMORANDUM FOR FILE

I. INTRODUCTION

This rationale was prepared in an attempt to provide a basis for assigning priorities to manned space flight experiments still in the definition phase. The objective is to insure that proper account is taken of the many aspects bearing on the potential worth and technical merit of each proposed experiment. Properly applied, such a rationale can be a valuable management tool in executing long-range plans. The experimental program is always subject to constraints which make it impossible to fly all the worthwhile experiments which are proposed. To optimize the allocation of money, mission payload and time, we require some knowledge of relative priorities at every stage, including that of early definition.

The definition phase of an experiment includes the period from acceptance of the initial proposal until completion of a detailed implementation plan. The experimental program up to five years in the future is essentially delimited by the experiments currently being defined. A well-ordered program will always have a relatively large number of experiments in this phase, and these experiments will be subject to a high mortality rate. Initially, the constraints of mission availability are very weak, and experiments are proposed and accepted mainly on the basis of their potential contribution to program goals. As time passes, mission schedules become firmer and new discoveries in science, advances in technology, or changes in program goals can all change the relative importance within any group of experiments. Priorities are, therefore, subject to drastic revision during this period. A rational basis for rating allows for rapid changes in ranking and easy identification of the reasons for a change.

The general approach and details of the rationale are described in the following sections. In the final section, a discussion is presented of our experience to date, some shortcomings of the method, and possible improvements and extensions of the rationale.

II. GENERAL APPROACH

A numerical rating for each experiment is derived from two separate factors, which we assume to be independent of one another. The first is a measure of the potential worth (W) or value of the experiment; the second factor is a measure of the technical merit (T). The final rating is the product of the two factors, $W \times T$. In order to obtain a high rating, therefore, an experiment must be highly rated for both factors.

Each of these factors, in turn, is derived from a number of subsidiary considerations. Thus, the potential worth comprises both the intrinsic merit of the proposed experiment and a priority weighting factor reflecting its relationship to current space-program goals and missions. The technical merit is based on consideration of the suitability for manned space flight*, likelihood of success and cost effectiveness of the proposed approach. The details of these considerations are described more fully in the next two sections.

Except for the priority weighting factors, which have been assigned separately, each experiment is rated for each characteristic on a scale ranging from 0 to 10 in integral steps. Zero is always the worst and 10 the best possible rating. The ratings in between have been determined mostly on an empirical basis, by discussions among the raters and relation to the overall rating procedure. The 0-10 scale is a generally familiar one, easy to apply and interpret. The establishment of appropriate intervals between the extremes of zero and ten, however, does present some scaling problems, which will be discussed later.

III. POTENTIAL WORTH

The potential worth \underline{W} is determined by first assigning each experiment to one of six priority groups, each group having a priority factor \underline{f} . The group definitions and priority factors used in the present study are given in the table below. Each experiment is also assigned a merit rating \underline{M} from 0 to 10, assuming that the experiment will be completed successfully. This rating is based primarily on judgment of the potential impact of the experiment in its field. The worth \underline{W} is then given by the product of \underline{M} and the priority factor \underline{f} , with the result rounded to the nearest integer.

* See comments on manned emphasis in last paragraph of this paper.

<u>Group</u>	<u>Priority Factor (f)</u>	<u>Definition</u>
I	5.6	Direct support of early Apollo missions.
II	4.0	Peripheral support of early Apollo, or for modified ALSEPs immediately following the first ten experiments.
III	2.8	Important experiments to prove man's usefulness <u>and</u> to fulfill a major program goal; both functions must be of major significance.
IV	2.0	Important <u>either</u> for proving man (or manned systems) <u>or</u> achieving other program goals; either must be of major importance and urgency.
V	1.4	Serving useful program goals, but not urgent.
VI	1.0	Minor contribution to program goals.

Priority Factors

The definition of these six groups and the assignment of priority factors are obviously subject to much discussion. Clearly, groups and priorities will change as the program evolves. But the present procedure can still be applied for any set of groups and priorities.

The actual priority factors, f , used here have been assigned in such a manner that each higher group improves the rating by about 40 percent. Thus, good experiments in a low priority group can surpass mediocre experiments in higher groups. The 40 percent spacing has been determined by trial and error to give a satisfactory degree of overlap between groups. A top-rated ($M = 10$) experiment, for example, has the same worth W as an experiment rated "7" in the next higher priority group, or one rated "5" two groups higher. The constant factor of upgrading per priority group as well as its value (1.4) are obviously crude approximations; but the resulting overlap does seem reasonable, whereas larger and smaller factors gave results which do not.

Program Goals

The major program goals considered in assigning these priorities include: lunar exploration; expansion of manned space flight capabilities; earth sciences; astronomy and astrophysics; life sciences; physical sciences; and advanced technology development. Except for the first two, no special priority has been given to any of these disciplinary objectives.

Lunar exploration is given special priority only for the purpose of maximizing the return from the nation's very large investment in early lunar landings. Experiments are assigned to groups I or II only if they provide direct or backup support to obtain results from scheduled missions. Lunar experiments not directly associable with these early landings are treated on the same basis as experiments in other areas. Many of the lunar experiments, of course, still have a high potential worth because of their contribution to expansion of manned mission capabilities.

Under expansion of manned space flight capabilities, we consider all biomedical and technological experiments related to increasing our capabilities for manned space flight. This category receives special consideration in that significant contributions in this area cause an experiment in any of the other areas to be placed in a higher group than similar experiments which do not contribute directly to manned space flight.

Earth science experiments are space-based studies of the earth and its atmosphere, including earth resources, meteorology and other important features which determine our immediate environment. The basic objectives are to advance our knowledge, to predict and control our environment, and to permit better use of our natural resources.

Astronomy and astrophysics provide a major scientific focus for the space program in the next decade. The ability to conduct observations above the interfering effects of the atmosphere constitutes a major breakthrough in these fields.

Life sciences objectives include biological studies not directly connected with expansion of our mission capabilities. Manned space flight provides unique opportunities for the study of the living state in a new environment and for the search for extraterrestrial life. These studies may also contribute indirectly to manned flight capabilities.

The physical sciences can also benefit from studies conducted in the unique space environment. Experiments in this area include studies of the behavior of fluids and crystals in zero gravity and tests of fundamental theories of relativity and gravitation. The radiation environment, particles and fields in space, properties of the ionosphere and auroral phenomena may also be included here, although there is some overlap with astronomy and earth sciences.

Advanced technology goals include development of communication, navigation and traffic control systems, advanced spacecraft subsystems, and any other basic or applied technology related to or benefiting from the opportunity for space

flight. Deployment of large space antennas, tests of advanced environmental control systems; studies of effects of space exposure of materials; and studies of atmospheric effects on communication systems are typical examples of activities in this area.

IV. TECHNICAL MERIT

We define the technical merit of an experiment to include those technical considerations lying outside of its potential worth or priority. The categories in which experiments are rated from 0 to 10 are:

S = Suitability for manned space program,

L = Likelihood of success, and

C/E = Cost effectiveness.

The suitability S^* depends on the necessity for a manned experiment in space. In addition, the question of whether the experiment makes good use of the man is considered. Further, the suitability is considerably reduced if the experiment imperils the astronaut or the mission in any way.

Likelihood of success takes into account the probability of obtaining all results stated in the objective, including problems of experimental complexity and past performance of similar experiments.

Cost effectiveness of an experiment should be found by dividing the probable return (potential worth times likelihood of success, or $W \times L$), by the cost in dollars, mission payload and astronaut time. A very expensive experiment may still be highly rated if it promises highly significant results and appears to be the most economical approach to its stated objective. At the other extreme, an inexpensive experiment may be poorly rated if it promises trivial results or imposes an excessive burden on the mission.

These categories are not independent of one another, and distinctions among them are often hard to make. An experiment low in suitability for manned space flight, for example, is likely also to be low in cost effectiveness. Similarly, an experiment with severe technological difficulties might be rated low in likelihood of success, low in cost effectiveness (because of heavy cost and long development time to overcome those difficulties), or both, depending on the optimism of the rater.

*See last paragraph of this paper.

The fact that correlations exist among these characteristics is not a particularly serious problem. The essential point in making a fair comparison is to make sure that all important features of the technical merit are evaluated uniformly for all experiments.

The technical merit T is expressed as the sum of S , L and C/E . The sum is used rather than the product largely because of the correlations among the three factors, but also because an experiment may still be desirable if only one of the three factors is very low. For example, an experiment which makes little or no use of man would be rated low in suitability for manned space flight, but might still be performed with high likelihood of success and inexpensively in a manned mission, giving it high L and C/E ratings.

In practice, the T values determined in this manner tend to have a relatively narrow spread. The dynamic range of T values in our first test of this method was only about one quarter of the range of W values. The rating of an experiment is thus usually determined mostly by its potential worth, with technical merit playing a relatively minor role. This is as it should be, considering the uncertainty in the technical merit at early stages.

V. SUMMARY OF THE RATING CALCULATION

For convenience, the numerical procedure for producing an overall rating is summarized briefly in this section.

The overall rating for each experiment is given by:

$$\text{Rating} = W \times T \text{ (maximum} = 1680\text{),}$$

where W is the potential worth:

$$W = f \times M \text{ (maximum} = 56\text{),}$$

f = priority factor (values of 1.0, 1.4, 2.0, 2.8, 4.0, and 5.6),

M = intrinsic merit rating (values 0 to 10),

and T is the technical merit:

$$T = S + L + C/E \text{ (maximum = 30),}$$

S = suitability for manned space flight (0 to 10),

L = likelihood of success (0 to 10),

C/E = cost effectiveness (0 to 10).

The entire rationale can thus be summarized in the single formula:

$$\text{Rating} = (f \times M) \times (S + L + C/E).$$

VI. DISCUSSION

Any numerical rating of experiments will always be open to criticism on some counts. However, when NASA faces the problem of selection from among several hundred candidates, some systematic procedure is imperative. If the system takes into account the most important considerations in a uniform way, it is likely to be far better than the judgments of various people made separately and with no uniform system.

We have thus far used this method in one preliminary trial, and in two full-scale applications upon a group of over 100 real experiments with a team of seven specialists in various disciplines doing the rating. It has been possible to integrate results from different raters with surprisingly few maladjustments. This is probably true only because the system was discussed thoroughly beforehand and the raters were therefore in rough agreement on the scale of rating and the definitions of the various categories. A final review of all ratings was made orally with all the team present to permit some adjustments. The resulting ratings were both reasonable and useful.

A table of the complete ratings for each experiment provides a great deal of information. In such a table, the specific reasons for a low or high overall rating are usually quite obvious. Reference to these values permits a reassessment of the experiment at a later date in case of changes affecting any factor.

Inherent Shortcomings

A basic question is the usefulness of any scheme to rank such a wide variety of experiments in a single ordered list. The problem arises from the variety of different fields represented and because of the great variation in costs of various

experiments. Furthermore, an experiment for which considerable definition effort has already been expended is subject to far more comprehensive review than one for which definition has not started.

A second inherent shortcoming is the small likelihood that a selection of only top-rated experiments will lead to a balanced experimental program. This is true of balance with respect to time as well as balance among various program objectives, since it is necessary to achieve a match between experiment availability and mission schedule. The adequacy of the program is also a problem. There is no guarantee that any selection, even if the whole list were included, would produce an adequate program in each field. Ranking the experiments in order will not correct weak areas in the original list. It may, however, be very useful in emphasizing such weak areas.

The problem of overlapping is a third serious drawback to any list of rated experiments. Examples include simple overlap between two experiments, cases where one experiment may be unnecessary if a certain group of other experiments is done, cases where a measurement may be useless unless certain others are made either previously or simultaneously, and cases where various combinations of these factors are present. The problem is that such relationships are not exhibited in a linear listing such as this. A great deal of relevant information about such overlap is uncovered during the rating process. Our practice has been to note this information in comments on the rating sheets. This has proved helpful, but not entirely satisfactory.

Possible Improvements

Possible mechanical improvements in this rationale are of three types: improvements and refinements of the rating categories; improvements of the rating scales within each category; and revision of the numerical procedures. While these will not be discussed here, a few remarks are important.

It would be a simple matter to renormalize the ratings to a range from 0 to 100, dividing W by 5.6 and T by 3. The result would be misleading, however, since an average experiment would then score less than 25 "percent" on this scale, instead of 50 "percent", due to the multiplicative process of the system.

An easily understandable rating would be obtained by converting the resulting list of all experiments to a percentile basis, or some other rating determined only by position relative to other experiments in the list. This would, however, lose

valuable information as to relative separation between experiments, for example, either very close, indicating near equality, or very far apart, indicating clear superiority of one experiment over the other. The present system demonstrates these separations nicely.

Hence, even though the resulting number scale of 0 to 1680 is not a familiar range, it was retained in this case and served very well in ranking all experiments.

Possible Extensions

Finally, we may consider two possible extensions of our rationale: first, inclusion of experiments already in the development phase; and second, extension to include unmanned as well as manned experiments. For development-phase experiments, more attention must obviously be paid to the technical merits, and more specific technical factors can be included. The suitability for particular missions will also usually be a factor. Otherwise, this extension can be made in a quite straightforward manner.

The present rationale is weighted heavily in favor of manned space flight experiments, both in its emphasis on manned space goals in the f factor and its consideration of suitability for man, in the T rating. A more comprehensive rationale would rank experiments in relation to NASA-wide program goals, and the suitability for manned space flight would be considered along with suitability for unmanned flight. Such a procedure would obviously be valuable in defining a balance between manned and unmanned missions resulting from the priorities of experiments in each and preventing unnecessary overlap between the two programs. It would also contribute to overall program adequacy by insuring that high priority experiments receive due consideration for manned missions if unforeseen circumstances prevent their flight on unmanned missions. Furthermore, there are many experiments of great potential value to the manned space program which now rank relatively low simply because they can be done more effectively in unmanned missions. These experiments are in danger of becoming orphans, neither suitable for manned flight nor contributing to unmanned goals. A broader rationale would assure proper consideration for such experiments.

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